It was, indeed, a fortunate circumstance that Darwin, after being repelled by the narrow and soulless system of "geognosy" taught by Jameson at Edinburgh, came at Cambridge under the spell of Henslow, a man of most catholic taste, extensive acquirements, and widest sympathy with all branches of natural science. By intercourse with Henslow, Darwin's flagging interest in science was rekindled and kept alive. It is a proud boast for a University to have nourished the intellectual development of Darwin; and as that University has in the past remained faithful to the memory of Newton, making his mathematical teachings the characteristic and leading feature of its studies, so, we may hope, it will in the future aim at that complete union of geological and biological investigation of which Darwin's labours constitute so grand an example.

In the dedication of his "Journal of Researches," Darwin acknowledged "with grateful pleasure" that "the chief part of whatever scientific merit this journal and the other works of the author may possess, has been derived from studying the well-known and admirable 'Principles of Geology';" and well do I recollect how, in almost every conversation I had with him, he would enlarge with warmth of feeling upon his indebtedness to Lyell, not only for his lucid teaching, but for his constant and helpful sympathy. How did he use to speak in terms of reverence of his "master," and extol the magnanimity of one who, though twelve years his senior, had abandoned slowly and cautiously, as was the habit of his mind, yet in the end completely and ungrudgingly, his own conclusions and prepossessions, and had accepted the doctrines of a pupil.

Of Darwin's three geological books, the record of the observations made by him during the voyage of the *Beagle*, it is impossible to speak in terms of praise that will seem, to those acquainted with the merits of those admirable writings, as too high; and some portions of those works, especially the chapters dealing with the great problem of foliation, are, I am convinced, very far indeed from having received from geologists the amount of attention which they deserve.

After Darwin's return to England, in 1836, his attention was for some years almost exclusively devoted to geological researches; and it was to this Society and to its officers that he constantly came for help, advice, and sympathy. He writes at this time, "If I was not more inclined for geology than the other branches of natural history, I am sure Mr. Lyell's and Lonsdale's kindness ought to fix me."

Before reaching England, Darwin had written to Henslow from St. Helena, on July 9, 1836, asking that he might be proposed a Fellow of this Society, and on November 30 of that year he was elected. In the following February he became a member of our Council, and at the next anniversary, in 1838, undertook the duties of Secretary. This office, after he had held it for five years, he was compelled to resign through ill health; but even after he had been driven from London through the same cause, it was the evening meetings of this Society which from time to time tempted him from the seclusion of Down, till at last painful experience proved to him that he must forgo even this too-exciting pleasure. Even after being compelled to lay aside his hammer, when he had taken up scalpel and microscope to study the Cirripedia, he did not forget the fossil forms of the same group.

Whether it was the distribution of organic forms in space, or the order of their appearance in time, which had had most to do in turning Darwin's thoughts into those currents which finally led him to evolution, it would be idle to speculate; but it may safely be asserted that the geological aspects of natural history had at least as much to do with the conception of the origin of species as had the biological.

How warm was Darwin's interest, all through his life, in the progress of every branch of geological research may be gathered from his letters to Lyell and other geological friends. In what he had a presentiment would be, and which actually proved, his latest work, "The Formation of Vegetable Mould through the Action of Worms," he returned in his old age to a geological problem which had occupied him during the years of his most intimate connection with our Society.

No memories can possibly have such fascination for myself as those of the conversations which, during the last seven years of his life, I was privileged to hold with Mr. Darwin upon the current topics of geological interest. It was his habit when he came to town, twice a year, to ask me to meet him, in order to talk over geological questions, and thus I had opportunities for close intercourse and discussion. No researches in our science were too minute, none too remote from the ordinary subjects of his study, to engage his attention and command his sympathies. How keenly did he recall the pleasures of his labours in this Society, and the happiness of the friendships which he had formed here! How generously and with what warmth of appreciation did he ever speak of the labours of those who had succeeded him in endeavouring to carry out the objects of this Society! Of the gentleness, the sympathy, the contagious enthusiasm of the man, I dare not trust myself to speak!

At a time when there is perhaps some danger that the excessive specialization which seems to have become a necessity in both the geological and the biological sciences, may lead to narrowness of view, restriction of aims, and petty jealousies among the workers in circumscribed departments of those sciences, it may be well to remember how Darwin, while engaged in the most minute and detailed investigations upon barnacles, earthworms, or pigeons, upon orchids, primroses, or climbing plants, could ever keep his mind open to the influence of each new discovery in every branch of geological and biological science.

The great principles which lie at the foundation of modern geology and of modern biology are the same; and Darwin did but furnish a new testament to the old covenant already accepted by geologists. Now, more than ever in the history of natural science, is there reason for the warmest sympathy, the most thorough understanding, and the completest union in effort between the cultivators of the geological and the biological sciences. It is not by petulant unfaithfulness to the tried methods of those two sciences, and a readiness to abandon the principles which have led us to such real and important conquests, for the older methods that have been so often discredited and found wanting, that we can hope to advance those sciences.

Lyell once wrote to Darwin as follows: "I really find, when bringing up my preliminary essays in 'Principles' to the science of the present day, so far as I know it, that the great outline, and even most of the details, stand so uninjured, and in many cases they are so much strengthened by new discoveries, especially by yours, that we may begin to hope that the great principles there insisted on will stand the test of new discoveries."

And to this Darwin replied with characteristic enthusiasm:—

"*Begin to hope?* Why the *possibility* of a doubt has never crossed my mind for many a day. This may be very unphilosophical, but my geological salvation is staked upon it! . . . It makes me quite indignant that you should talk of *hoping.*"

Fifty years have elapsed since these words were written. How infinitely more complicated seem to us the problems involved in the explanation of the past by the study of the process going on around us at present, than they possibly could have done to the great pioneers of the Uniformitarian doctrines! But the reasons for Lyell's hope and Darwin's confidence are still valid, nay, are stronger than ever. For does not every new discovery remove some difficulty or supply fresh illustrations of these views? May every geologist to-day be endowed with a due share of Lyell's caution; but, for my own part, I see no reason why he should not also possess a full portion of Darwin's faith.

ON THE NUMBER OF DUST PARTICLES IN THE ATMOSPHERE.[1]

AT the beginning of the paper, reference is made to the great advance recently achieved by physiologists, regarding our knowledge of the solid matter floating in the atmosphere, as they have already provided us with a considerable amount of information regarding the number of live germs in the air under different conditions; while we have but little information regarding the dead organic and inorganic particles. The following investigation was undertaken in the hope of bringing the physical side of the subject abreast of the physiological; and in this paper is given an account of a method devised by the author for counting the dust particles in the air, and also some results obtained by means of it.

One difficulty presented in this investigation is the extreme minuteness of the particles to be counted; most of them are not only invisible, but are beyond the highest powers of the microscope. It was therefore necessary to adopt some method of making them visible. The simplest plan of doing this is to put the air—the particles in which we wish to count—inside a glass receiver, and saturate it with water vapour; then to supersaturate the air by slightly expanding it by means of an air-pump. When this is done, a fog is produced in the receiver, and we know that each fog particle has a dust particle as a nucleus; if then we counted these fog particles we would get the number of the dust particles. By this process, however, we would not by any means have counted all the dust particles present, as the fog particles so formed do not represent nearly all the dust particles. If, after time has been given for these fog particles to settle, another supersaturation be made, the receiver will become packed with another set of fog particles, which would require to be counted; and this process would require to be repeated a great number of times before the last particles would become visible and be counted. It is then shown that if there is only a little dust in the air, so that the particles are wide apart, then only one supersaturation is required to make all of them visible. Further, when there are few dust particles present the fog particles are large, and are easily seen falling like fine rain inside the receiver; and it appeared that if these rain drops could be counted then the solution of the problem promised to be easy.

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[1] Communicated by permission of the Council of the Royal Society of Edinburgh, having been read to the Abstract Society on February 6, by John Aitken, F.R.S.E.

The following gives a general idea of the method adopted of working out this suggestion. A small glass receiver was connected on the one side with an air-pump and on the other with a cotton-wool filter. Inside the receiver was fixed a small stage, about 1 cm. square, on which the drops were to fall and to be counted. This stage was fixed at a distance of 1 cm. from the top of the receiver, it was ruled into little squares of 1 mm., and was examined through the top of the receiver by means of a magnifying glass. To illuminate this stage a gas flame was used, the light being concentrated on it by means of a globular lens full of water. The air in the receiver was pumped out, and filtered air admitted. This air was perfectly dust-free, and gave no condensation when expansion was made. Into this pure air was admitted a small and measured quantity of the air the particles in which we wished to count. After allowing a short time for the air to get saturated, one stroke of the pump was made, which supersaturated the air, and brought down a shower of fine rain; while making the stroke with the pump, the stage was carefully observed through the magnifying glass, and the number of drops that fell on a square millimetre counted. This was repeated a number of times, and the average number of drops per square millimetre was obtained, and used for calculating the number of particles in the air. For every drop that fell on the square millimetre, 100 fell per square centimetre; and as there is only 1 cm. of air above the stage that number will represent the number per cubic centimetre in the air of the receiver. Then, knowing the proportion in which the air tested was mixed with pure air, and knowing also the amount to which the air was expanded by the pump, we have all the figures necessary for making the calculation of the number of particles in the air under examination.

In constructing the apparatus the first thing to which attention was given was to design the arrangement of stage or platform on which the drops could be most easily seen and counted. The first stage tried was a small piece of glass mirror, ruled on the back into little squares. This seemed at first to give excellent results, the drops being easily seen on its surface; but on attempting to count them its unsuitableness was at once evident—the confusion produced by the reflected images of the drops caused it to be abandoned at once. Then a mirror of very thick glass was tried, the glass being so thick that the reflected images were out of focus, but it did not give satisfactory results. Very thin mirrors made of microscope glass were then tried, but had to be rejected, because, though they brought the drops and their reflections together, they were unsuitable, being too rough and covered with fine specks on their surface; only the most highly finished glass can be used for this purpose. The arrangement was then altered, and a transparent stage lit from beneath was tried. This stage was made of a small piece of carefully selected glass, and had the fine lines etched on its surfaces. It was, however, abandoned, as it did not give such promise as the mirror arrangement. All difficulties in the use of mirrors were at last got rid of by making them of silver, and now silver mirrors are the only kind used. They are very highly polished, care being taken to keep the rubbing marks in straight lines and in one direction; they are ruled with fine lines at right angles to each other and at 1 mm. apart. When a silver mirror is mounted in its place, properly adjusted and lighted, it appears, when seen through the lens, like a black surface on which the lines are quite distinct, and on which the small drops shine out brilliantly and are easily counted.

Some difficulty is experienced in keeping the stage at the proper temperature. If it is too hot, the drops on falling on it do not adhere, but present a beautiful illustration of the spheroidal condition, as they roll over its surface towards the lower side of the stage, and drop into the ruled lines, in which they continue rolling till quite evaporated. On the other hand, if the stage is too cold it gets dewed, and counting becomes impossible. Directions are given in the paper for mounting and keeping the counting stage in the best working condition.

In working the apparatus two methods have been employed of mixing the air to be tested with dust-free air. In one, the dusty air is introduced into a flask which communicates with the test receiver by means of a pipe provided with a stopcock. The small quantity of air that is to be mixed with the pure air in the receiver is displaced from this flask and driven into the receiver by means of a carefully measured quantity of water which is run into the flask. In this way the air to be tested can be measured with a fair degree of accuracy, and as the capacity of the receiver is easily obtained, the experimental errors need not be great.

In the other method of working, the test receiver is connected with a small gasometer, and the air to be tested is mixed with pure air in the gasometer. The gasometer used has a capacity of 20 litres, is carefully graduated and delicately hung, so that the air can be measured in it with a considerable degree of accuracy. In working this arrangement, 1 litre of the air to be tested is generally first mixed with 19 litres of filtered air. After mixing, nine-tenths of the mixture is let out, and the gasometer again filled up with pure air, and the mixture tested in the receiver. If the drops are still too close, more air is let out, and filtered air added till the desired condition is attained. There must not be too many particles present, or all of them will not fall when expansion is made. Till experience is gained, a check on the quantity is easily obtained by admitting filtered air, in place of the air from the gasometer, and seeing if any drops appear on expansion; if none, then the correct number has not been exceeded.

After a satisfactory counting stage had been devised, and the apparatus got into working order, testing began, when at once difficulties presented themselves. The numbers counted in the successive tests of the same air agreed fairly well for a number of times, then all at once the process seemed to break down, and from time to time a great increase in the number was counted, far exceeding the errors of experiment; then all would go right for a time, but only to be followed by failure before long. The first thing suspected for these and for other failures was always the joints of the pipes and the stopcocks, and time after time have the joints been remade with india-rubber solution and stopcocks cleaned and greased, but to find that they are almost never at fault.

It was then suspected that the failure might be due to the filtered air, with which we mixed the dusty air, not being perfectly freed from its dust. The filtering power of cotton-wool was therefore studied, when it was found that 1 inch of cotton-wool will filter perfectly if the air is passed very slowly through it, but that even 12 inches of cotton-wool will not check all the particles if the air is made to rush violently through it. Filters must therefore be tested under exactly the conditions in which they are to be used.

It was, however, found that though the air was only allowed to pass very slowly through even 12 inches of cotton-wool, condensation frequently took place if the expansion and therefore the supersaturation was great. It was thought that in this case the failure might be due to an imperfect action of the filter—that, while it checked most of the dust, yet it allowed the extremely small particles to pass, and that these extremely small particles became active centres of condensation when exposed to the high degree of supersaturation used in the tests. It therefore here became necessary to test whether the *size* of the particles has *practically* any effect on the degree of supersaturation necessary to cause the vapour to condense on them. From the investigations of Clerk Maxwell we have theoretical reasons for expecting that the size of the particles will have an influence of this kind, but at present we cannot say that it is sufficiently great to have a perceptible effect in an experiment such as that described.

To test this point the following experiment was therefore made. A little dusty air was mixed with filtered air, and put into the test receiver, and saturated with water vapour. An expansion of only 2 c.c. was made; this caused the formation of a fog. After these fog particles had settled, the air was returned to the receiver; and after a short time another 2 c.c. expansion was made, when other fog particles appeared. After this had been done a number of times, the density of the fog got less and less, and at last entirely ceased. After this an expansion of 5 c.c. was made; this produced a rainy condensation in the receiver, which appeared a number of times on successive expansions being made, getting less and less dense, and at last it also ceased entirely. After all condensation had stopped with the 5 c.c. expansion, the expansion was increased to 10 c.c., when another shower made its appearance, and after one or two expansions all condensation again ceased. After this condition was attained, an expansion of 150 c.c. was made with the pump, when scarcely one drop made its appearance.

It is concluded that in the above experiment we have distinct evidence that the *size* of the particle does affect the degree of supersaturation required to produce condensation on it. Because, though an expansion of 2 c.c. produced a supersaturation sufficient to cause more than one-half of the particles to become visible, yet it required a higher degree of supersaturation to cause condensation to take place on others. It is also concluded from the experiment that the failure of the air to keep clear, in the tests where high supersaturation was used, was not due to the presence of extremely small particles, as an expansion of 10 c.c. is practically great enough to produce a supersaturation sufficient to cause condensation on the smallest particles.

The failures in the tests not being due to the presence of extremely small particles, it is concluded that they are true cases of condensation without nuclei, similar to those referred to in a previous communication. It was thought that, if they were true cases of spontaneous condensation, they might be checked if the expansion was made slowly and free from shocks. And on the other hand any shock would tend to produce condensation in dust-free air if highly supersaturated. On trying this, it was found that no condensation took place if the stroke of the pump was made slowly and steadily, and that if done quickly, and the piston made to strike the cover of the cylinder violently, then copious showers were always produced in the dust-free air. Here, then, was the key to one of the difficulties, and accounted for the occasional increase in the number of the particles counted; many of the drops having no dust-nucleus. Failure from this cause is now entirely prevented by causing the air on its passage from the receiver to the pump to pass through a small opening, or better through a small cotton-wool filter; this checks all violent rush of air, and shocks, and keeps the filtered air perfectly free from condensation even when highly supersaturated.

Again, the failure of perfectly filtered air to keep free from condensation was frequently observed after the inside of the test receiver had been newly wetted. It looked as if the newly wetted sides had saturated the air more thoroughly, and that the condensation was due to the higher degree of supersaturation which took place when expansion was made. This class of failures was, however, traced to the manner of wetting the inside of the receiver. If it was done roughly, and the water splashed, then many nuclei were manufactured in the receiver; if it was done quietly, none, and no condensation followed. Another cause of failure was traced to a drop of water getting into the pipe by which the air entered, and the inrush of air tearing the water into fine spray, which became active centres of condensation.

As yet no great number of tests of air have been made under different conditions, natural or artificial; but in the following table will be found some of the results obtained by this method of counting.

Dust Particles in the Atmosphere.

Source of the Air.	Number per c.c.	Number per c.in.
Outside Air—Raining	32,000	521,000
,, ,, —Fair	130,000	2,119,000
Room	1,860,000	30,318,000
,, near ceiling	5,420,000	88,346,000
Bunsen Flame	30,000,000	489,000,000

In the first column of the table is entered the source of the air; in the second, the number of particles per cubic centimetre; and, for the benefit of those who think in English measures, the number per cubic inch is entered in the third column. The first number in the table, for ordinary outside air, was obtained on January 25, after a wet night. The number given for fair weather is an average got when the weather was in that condition. As yet far too few measurements have been made to enable us to trace any connection between the number of particles and the weather, but it is hoped that something practical may result from observations of this kind. The first number given for the air in a room is the number counted in the air of a room where gas was burning, and taken at a height of 4 feet from the floor; the other number was counted in air drawn from near the ceiling; and the last number was got in the air collected over a bunsen flame.

The value of numbers given in the table has not been carefully considered, and they are not given as absolutely correct; great accuracy, indeed, does not seem possible, when we consider the conditions; and, further, the number is constantly varying. For this reason it has not been thought necessary to make any corrections for temperature and pressure. Though we can get with a fair degree of accuracy the number of particles in the air in the test receiver, yet in all probability the calculated numbers given in the table are rather under than over estimates, as it is difficult to manipulate air without losing much of its dust. For instance, in one hour about one half of the particles settle out of the air in the gasometer. Though the numbers do seem very large, yet so far as can be judged at present they are fairly correct, and at least represent the kind of numbers we have to deal with. It does seem strange that there may be as many dust particles in 1 cubic inch of the air of a room at night when the gas is burning as there are inhabitants in Great Britain, and that in 3 cubic inches of the gases from a bunsen flame there are as many particles as there are inhabitants in the world.

JOHN AITKEN.

UNIVERSITY AND EDUCATIONAL INTELLIGENCE.

CAMBRIDGE.—The Frank Smart Studentship of Botany, founded at Gonville and Caius College by Mr. F. G. Smart, M.A., M.B., and Mrs. Smart, by the transfer of £2400 Great Eastern Railway 4 per cent. Debentures, is to be awarded for the first time at the beginning of Easter Term. Candidates are to send in their names to the Master of the College, Dr. Ferrers, on or before March 20. The electors are the governing body of the College, acting after consultation with the Professor and the Reader of Botany for the time being in the University. The Studentship is to be open to all members of the University who have taken honours in the first part of the Natural Sciences Tripos, and of not more than five years' standing; but the elected Student must become a member of Gonville and Caius. No competitive examination is to be held for awarding the Studentship. The Student is to apply himself to original investigation in botany, and must be able to show that he is doing so at any time, on penalty of forfeiting the Studentship. The Studentship is to last two years, but may under special circumstances be prolonged for one year more. The regulations of the Studentship are only to be changed, after the death of Mr. and Mrs. Smart, by consent of the Council of the Linnean Society of London. A prize of £6 in books is to be given out of the interest of the fund to the undergraduate student of Caius College who shall distinguish himself most in botany at the annual College examination.

The collection of British birds' eggs made by the late Mr. J. P. Wilmot, of Trinity College, containing a specimen of the great auk's egg, and other specimens figured in Hewitson's "British Oology," has been presented to the University by Lady Caroline and Mr. C. H. Russell, in memory of Mr. George Lake Russell, Lady Caroline's late husband.

Plans are submitted for the proposed new plant-houses at the Botanic Garden, to cost £2760, and of a laboratory in the garden, to cost £250.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, February 9.—"The Small Free Vibrations and Deformation of a Thin Elastic Shell." By A. E. H. Love, B.A., Fellow of St. John's College, Cambridge.

In this paper the method employed by Kirchhoff and Clebsch for the treatment of a thin plane plate is applied to the case of a